ences. Dr. Fleming also proposes the name "kumascopes" for all forms of Hertz wave detectors, but it is not a very pleasant sounding term.

Telegraphy over wires was not neglected by the congress, and Dr. Kennelly gave both an excellent theoretical paper on the transmission speed over submarine telegraph cables, and a practical one on high frequency telephonic circuit tests. To test the telephone circuit for effectiveness a known sinusoidal E.M.F. is applied, and the corresponding received current strength is measured; the ratio of these quantities Dr. Kennelly calls the "receiving end impedance" of the circuit at the frequency used (600 ~ in the tests). If this impedance exceeds a certain value, then the circuit will be defective or inoperative. The interesting part of the apparatus is that used for the measurement of the received current; this is accomplished by passing it through a small platinum wire (Fessenden barretter), which it heats, and the change in its resistance is measured.

By this means, using a 3 micron wire, 23 microamperes can be measured, and with a 1·7 micron wire *in vacuo* 3 or 4 microamperes is said to be measurable. For practical tests on telephone switchboards the use of a sensitive reflecting galvanometer, which the above arrangement involves, is not very convenient, so the change in resistance of the platinum wire is observed by putting it in series with a sensitive milliameter and cell. A complete portable apparatus of this kind was described, with which one scale-division change in deflection of the Weston milliameter corresponded to 1·4 milliamperes of superposed alternating current. Curves are given in the paper showing tests of different lengths of cables.

The improvement of telephonic communication by increasing the self-induction of the circuits is receiving considerable attention in the States, and Dr. Hammond Hayes gave some most striking curves illustrating the reduction in attenuation which has been produced by the use of uniformly spaced loading coils on long circuits. The improvement is very much more marked in the case of cables than air wires. The most striking results are those obtained with a standard telephone cable which was heavily loaded so that the added inductance amounted to about 0·6 henry per mile. In this case, from Dr. Hayes's curves the received current was reduced to about ½ per cent. of the transmitted value at a distance of fifty miles with the cable unloaded, whereas with the loaded cable the received current was 7 per cent. Further, the great importance of terminal reflection where the loaded cable joins the transmitting and receiving apparatus is most marked, as by reducing the self-induction of the end loading coils so as to taper it off and avoid a sudden change in the self-induction the received current was increased to about 18 per cent.

It is also very interesting to note how the curves cross one another, so that short lengths of cable give better results without loading, whereas the cable with loading and terminal taper above six miles long produces less attenuation than the unloaded cable, the advantage in favour of the loaded cable increasing with its length.

There were many other papers of great scientific interest; among these may be mentioned Dr. Pender's paper on the magnetic effect of moving charges, which clears up many of the differences which existed between his results and those obtained by Crémieu, and suggests several other interesting problems; Prof. Wilson on condensation nuclei; two papers on the theory of conduction by Prof. Drude and Prof. Richards; and Prof. Arrhenius's paper on the electric charge of the sun.

In conclusion, it must be said that the congress was a complete success, perhaps more so than might have been expected, considering the great distance many of the members had to travel to attend its meetings, and this was greatly due to the indefatigable energy of its organisers, and especially to Prof. Elihu Thomson, the president, Dr. Kennelly, and Mr. Weaver. The attendance at the meetings was good, and if the discussions were not always as full as could be wished, this was not from lack of interest in the papers, but from lack of time. All the foreign members of the congress, irrespective of nationality, were received and entertained in the most hearty manner by their American *confrères,* fully bearing out the world-wide reputation that America has for hospitality.

W. DUDDELL.

PHYSIOLOGICAL CHEMISTRY IN THE UNIVERSITY OF GLASGOW.

TOWARDS the close of his introductory lecture to the course of physiology in the University of Glasgow on October 13, Prof. McKendrick said:—

I think there can be little doubt that the next great advance in physiology will be from the side of physiological chemistry. The phenomena of vital activity depend on chemical processes in which there are either the building up of complex substances by the union of simpler ones, or the decomposition of complex bodies into simpler ones—in other words, processes that are of a synthetical or of an analytical nature. These chemical phenomena lead, on the one hand, either to the locking up, or, on the other, to the liberation of energy, and the energy in a living being may appear as mechanical motion, heat, electricity, and to some small extent, and in special cases, as light and sound. During the last sixty years many of the physical phenomena of the living being have been investigated by special methods. It seems to me that we cannot expect much more from the application of the graphic method of registration, nor from the examination of the phenomena of electrical action in living tissues. The microscope and the methods of histological research have left little to be desired as to our knowledge of the structure of the elementary tissues and the structure of organs. A new departure must be made. No method of research seems so inviting or so promising as the rigid and methodical investigation of the chemical phenomena happening in living matter.

Hence the extreme importance of the chemist and the physiologist working hand in hand for the future advancement of physiological knowledge. At one time it was supposed that the chemical phenomena happening in the living body were of a different order from those occurring in dead matter. In 1824, however, Wöhler pointed to the first example of a synthetical process discovered within the animal organism. He showed that when benzoic acid is introduced into the stomach it appears as hippuric acid in one of the excretions, after coupling, probably in the liver, with amido-acetic acid or glycocoll. About the same time Hennell effected the synthesis of alcohol, and Wöhler formed urea from ammonium cyanate. As urea was then known only as a product of the animal organism, its synthesis from inorganic substances, and in the laboratory, was a feat of the first importance. This synthesis was the precursor of many others, so that we have now, at the lowest estimate, between two and three hundred chemical substances found in plant and animal tissues that can also be built up synthetically by the organic chemist. Year by year we are adding to this extensive list. Some of these syntheses are striking examples of the knowledge and skill of the chemists of the present day. Such, to mention one brilliant series, are the artificial productions of the sugars by the labours of Fischer and his pupils. Take, again, the formation of the highly complex body camphor, realised by Komppa and Vorländer. It may not be a day-dream if we contemplate the time when even the starches, fats, and proteids we use in our food may also be artificially formed. Physiological chemists have also done much in the way of studying the chemical changes happening to a substance during its passage through the body, but this is a much more difficult branch of physiological chemistry than even the synthetic production of organic bodies.

And yet we are far from solving the mystery of what we may call vital chemistry. When we think, for example, of the synthetical processes by which the chemist constructs complex bodies hitherto only found in the tissues of plants and animals, the question naturally occurs: how does nature produce these complicated molecules without the use of strong reagents and high temperatures? This aspect of the question has been well discussed by my friend Prof. R. Meldola, first, in an address as president of the chemical section of the British Association at the Ipswich meeting in 1885, and, second, in an important work, soon to be published, the proof sheets of which he has kindly allowed me to peruse, entitled "The Chemical Synthesis of Vital Products." It is clear from a study of the examples given by Prof. Meldola that the synthetical processes worked out by the chemist in his laboratory are quite unlike those occurring in plant and animal tissues, and yet the result is the

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same, namely, the production of a complex organic compound. We have been too much in the habit of supposing that when we could represent the process followed by the chemist by an equation, that this equation represented what occurred in the plant or animal tissue. Physiologists more than chemists have erred in this direction, and many of the statements in our text-books are either superficial or grossly misleading. The chemist attains his end by violent means and with considerable rapidity, whereas, in the silent laboratory of the plant and animal cell, molecular processes are slowly carried on of which we know at present next to nothing. It is strange, for example, that we cannot yet follow all the steps of the process by which, under the action of sun-light, the green colouring matter in a vegetable cell can fix the carbon of the carbonic acid of the air and liberate the oxygen. Nor can we follow satisfactorily the steps of the synthesis by which the carbon is built up into such a substance as starch or sugar. Yet this is a synthesis accomplished every day by every green plant. Such phenomena in all probability are accomplished through the agency of enzymes or ferments, but their real nature is still obscure.

I have said enough to show you the vast importance of chemical investigation in the physiology of the future. Chemistry is but a highly specialised branch of physics. In these days all the new discoveries in physical chemistry, such as the true nature of solution, the facts of dissociation as exemplified by such a common phenomenon as the splitting up of common salt into the ions chlorine and sodium, the charging of each ion during electrolysis, and the laws of osmotic pressure, which no doubt regulate nutrition and the interchanges of blood and lymph, must be taken into account by the physiologist. Such research demands adequate laboratory accommodation and highly trained specialists. I am glad to say our university will soon be in a position to take her share in this new development of physiological science. The splendid laboratories now being built for physiology, public health, and materia medica will be a home for work of this kind, and the endowment of a lectureship in physiological chemistry by the trustees of the late Dr. John Grieve (who left 8000*l.* for the foundation of a lectureship in connection with the medical faculty of the university) will enable us to obtain the services of a trained specialist, who will give his undivided attention to this department of physiology. No subject more than physiology illustrates the truth that all science is one. Physics, chemistry, physiology, and all the others are only different ways of investigating the phenomena of nature. The phenomena of life are, however, the most difficult of all to investigate, and it may safely be asserted that the highest skill in experimental research and the deepest knowledge of chemistry and physics are required for such work. Throughout the scientific world physicochemical researches are now in progress into physiological and bacteriological processes, lectureships and laboratories are springing up here and there, and it is gratifying to be assured that the University of Glasgow will be able to take her share in this work.

CONDENSATION NUCLEI.[1]

A FAMILIAR experiment was first shown illustrating the action of ordinary dust particles as condensation nuclei. From a large globe, which had been allowed to stand for some hours, some of the air was removed by opening communication with an exhausted vessel. Only a very few drops were formed as a result of the expansion. On allowing air to enter the globe through a cotton-wool filter, so that the pressure was brought back to its original value (that of the atmosphere), and allowing the air to expand as before, the drops formed were again very few. The ordinary air of the room was now admitted; an expansion of the air in this case resulted in the production of a thick fog.

When air has been freed from dust by filtering, or by repeatedly forming a cloud by expansion, and allowing it to settle, the vapour which, in the presence of the nuclei, would have separated out in drops, must be in the "supersaturated" condition immediately after the expansion is completed.

Another method of producing clouds was now shown. Air was allowed to escape through a fine orifice into an atmosphere of steam; the mixed air and steam were then passed through a Liebig's condenser, where the greater part of the steam was condensed, and then into a large glass globe, where the clouds were observed. From this vessel the air was drawn off by a pump which maintained the pressure in the globe and condenser at a considerable number of cms. of mercury below that of the atmosphere. Before reaching the jet the air of the room had to pass through a cotton-wool filter, and then through a long tube containing water; finally it was led through an aluminium tube to the orifice. The latter was about half a mm. wide. The fall of pressure in passing through the orifice was about 15 or 20 cm. In the absence of the filter, the air being admitted directly to the water tube through a tap turned just sufficiently to give the same flow as with the filter, a dense fog poured out from the end of the condenser tube; on closing the tap and letting the air enter through the filter the fog rapidly cleared, and only a fine rain continued to be produced. While the apparatus was in this condition an X-ray tube was set in action near the aluminium tube; the rain was succeeded by fog, which continued to pour out from the end of the condenser so long as the X-rays were kept in action. Condensation nuclei are, as this experiment proves, produced in air exposed to Röntgen rays. Later experiments will, however, show that they have entirely different properties from the ordinary dust nuclei.

When air has been completely freed from dust particles, so that a slight expansion of the air (initially saturated with water vapour) does not result in the formation of any drops, it is found that quite a high degree of supersaturation may be brought about without the appearance of a single drop. There is, however, a limit to the supersaturation which can exist without condensation of the vapour in drops resulting. To study this condensation in dust-free air, and to measure the expansion required to produce the necessary degree of supersaturation, a special form of expansion apparatus is required. The lantern slide thrown on the screen shows the construction and mode of working of the apparatus. The second slide is a photograph of the machine in action, the exposure having been made immediately after an expansion; the cloud formed (in this case on nuclei produced by the action of radium) is plainly visible along the path of a concentrated beam of light from a lantern.

Let us now try an actual experiment with the expansion apparatus. On making a slight expansion a cloud forms on the dust particles which are present; this slowly settles to the bottom of the vessel. The air is allowed to contract to its original volume, and a second expansion of the same amount is made. The drops formed are on this occasion comparatively few, and they fall rapidly; the dust particles have nearly all been carried down with the drops formed by the previous expansion. The fewer the nuclei on which water condenses the larger will be the share of water available for each drop, and the more rapid will be the fall. The next expansion produces no drops. While the air is in the expanded condition, the piston being at the bottom of the expansion cylinder, air is removed from the cloud chamber by opening the connection to the air-pump until the pressure is about 13 or 14 cm. of mercury below that of the atmosphere; the piston is again allowed to rise by putting the air space below it in communication with the atmosphere. The next expansion is thus comparatively large, the pressure after the expansion has taken place and the temperature has risen to its original value being 13 cm. or more below the initial pressure. Yet, in spite of the high degree of supersaturation reached, not a drop of water is seen. Making the fall of pressure 16 cm., however, we see on expansion a shower of drops; and although these drops are few and large, falling therefore rapidly, yet, however often the same expansion be repeated, the drops produced on expansion show no diminution in number. Thus the nuclei removed with the drops are continually replaced by others manufactured within the apparatus itself.

To produce the necessary supersaturation to cause condensation in the form of drops in dust-free air, the air must be allowed to expand suddenly until the final volume is 1·25 times the initial volume. The condensation is rain-like in form, and, moreover, the number of drops remains small although the expansion considerably exceeds this lower limit.

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[1] Discourse delivered at the Royal Institution on Friday, February 19, by C. T. R. Wilson, F.R.S.

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